SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

Published at

229 BROADWAY, NEW YORK.

P. O. Box 3838.

SATURDAY, AUGUST 28, 1880.

The annual session of the American Association for the Advancement of Science has been most brilliantly opened in Boston. The intellectual force now concentrated there will soon be flowing through all the channels of knowledge. Our columns next week will contain our reporter's account of the proceedings, and will be enriched by an address, in full, of the distinguished retiring President, Professor George F. Barker, whose learning and devotion to Science alone placed him in that elevated position. We have also obtained valuable and interesting papers by Professors Agassiz, Hall of Washington, and other distinguished participants, which will duly appear.

Mr. Paget Higgs, the well-known English electrical engineer, now in Boston, has given his opinion, through the New York *Herald* (August 27), on the durability of electric motors and their actual return in work. As the general introduction of Edison's electrodyramo-machine is being anxiously looked for wherever a constant supply of cheap power is necessary, it becomes of the first importance to consumers to know how long the new engines will last. Mr. Higgs' positive statement of their length of life will no doubt confirm many small manufacturers in New York in their intention to profit by this convenient source of power, which, rumor says, will soon be generally placed at their disposal. Mr. Higgs has run some of the older and less perfect electro-motors since 1867, and finds them to-day in perfect condition. As the fruit of his own experiment and observation of the work of the most experienced European electricians, Mr. Higgs emphatically denies that there is any extraordinary loss in using them to communicate power at a distance.

We drew attention to an educational scheme which has been recently inaugurated at the Paris Observatory for the purpose of training young astronomers. It may be interesting in this connection to know that Professor Stone, of the Cincinnati Observatory, has for a number of years been quietly but successfully pursuing a plan in almost every respect identical with that more recently inaugurated in Paris. A small number of selected graduates are admitted as students at the Observatory, pursue a systematic course of study in theoretical and practical Astronomy, and upon its successful completion receive a post-graduate degree from the authorities of the University.

The course of study carried on at the Paris Observatory is described in Science, August 14th. If there are other Observatories in the United States offering the same facilities as those initiated by Professor Stone, we shall be glad to hear from those who can give authentic information.

We are not surprised that universal regret is expressed at the loss by the New York Fishery Commission of their annual appropriation. It appears to be acknowledged that the Commission was doing good work, and we trust their present difficulties are but temporary, and will be removed when the matter can be considered by the Legislature.

We think the Commissioners would strengthen their hands in efforts to obtain a renewal of their appropriation, if they gave some attention to the coarser kinds of fish, the supply of which appears to be practically unlimited at our very doors, and yet for unaccountable reasons is retailed at exorbitant prices, even averaging that of meat.

Fish is a natural food product for the poor of cities situated on the coast, but the dealers combine to make it an expensive luxury, by limiting the supply. We are even told that they destroy it, rather than effect sales below the prices they have arbitrarily fixed.

There appears to be little encouragement for the Legislature to grant appropriations to increase the supply of fish and lower its price, if the dealers in combination have finally the power to limit the supply and to create an artificial value.

As one of the New York Fishery Commissioners is himself one of those who are most largely interested in the sale of fish, his knowledge on the subject must be considerable, and he would certainly promote the interest of the Commission by assisting to remove the evil of which we complain. While it may be a good work to load the table of the epicure with choice fish, it should be more satisfactory to restore to the poorer classes an article of food which nature has supplied with such a bountiful hand.

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ELECTRO-MOTORS.

THEIR POWER AND RETURN.

J. HOSPITALIER.

The transmission of force from a distance, electric ploughing, the electric railroad, etc., have made electric motors and the conditions of maximum work and maximum return, quite the order of the day. In a previous article on the available force in batteries, we have determined, for the most usual forms, the quantity of energy that could be furnished by a certain number of elements in an external circuit of proper resistance, supposing no polarization and without variation of the internal resistance.

Is this maximum of available work entirely convertible into effective work? It is not, and we will show how this maximum should be reduced when a given electric energy is to be transformed into mechanical force.

Let us suppose, for instance, in numbers, which always strike the attention more than formulas, that we have a source of electricity of 100 volts, with an internal resistance of 1 ohm. It would be easy to realize the conditions by employing an electro-dynamic machine, separately excited, or 100 very large Bunsen cups, arranged for tension in 2 parallel series of 50 each. Putting into the circuit an external resistance equal to the internal, and supposing no polarization to exist and no change in the internal resistance, we obtain as elements for the electric circulation:

E.—Electro motive force	=	100 volts.
r.— Internal resistance	=	1 ohm.
R.—Exterior resistance	=	1 ohm.
$(r + \text{R})$—Total resistance	=	2 ohms.
Q.—Quantity $\dfrac{\text{E}}{r + \text{R}} = \dfrac{100}{1+1}$	=	50 webers.

In these conditions we know that we have in the external circuit the maximum of available work, as deduced from the formula of Joule:

$$\text{W} = 10\ \text{Q}^2\text{R meg-ergs} \qquad (a)$$

$$\text{or W} = \frac{\text{Q}^2\text{R}}{9.81} \text{ kilogram-meters} \qquad (b)$$

In the case before us we have:

$$\text{W} = 10 \times 50^2 \times 1 = 25{,}000 \text{ meg-ergs} \quad (1)$$

What can we do with this available electric work? If we make it traverse an inert wire it will heat it. All the electric energy will be transformed into heat, and in this wire will be developed a certain number of calorics C, per second:

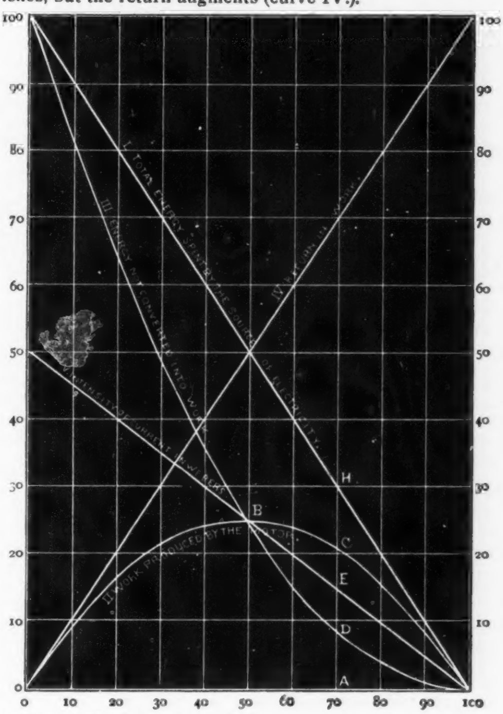
$$\text{C} = \frac{\text{Q}^2\text{R}}{9.81} \times \frac{1}{\text{A}}. \qquad (c)$$

A being the mechanical equivalent of heat 424.

Let us substitute for the inert resistance of a wire, an electro-motor of equal resistance with the wire, say 1 ohm in this particular case. Let us suppose this motor to be one of Gramme's magneto-electric machines, and that the resistance of the armature is equal to 1 ohm. If we put a break on the armature to prevent it turning under the influence of the passing current, we will not have any of the original conditions changed; the wire of the armature will be heated by the current, and a number of calorics C will be produced equal to that developed in the wire. Now let us make the armature turn under the action of the electric current. The rotary motion of this armature will develop a certain electro-motive force E′, inverse to that emanating from the source of electricity E, varying with the speed of the motor. It results in a diminution of the current, and can be expressed at each instant by the formula:

$$\text{Q}^1 = \frac{\text{E} - \text{E}'}{r + \text{R}}. \qquad (d)$$

Hence the rotation of the motor diminishes the intensity of the current (and consequently the work of the motor) if a machine is employed as a source of electricity, or the consumption of zinc, if you employ a battery. The diagram shows how the different elements vary when the speed of the motor varies from zero (where the work developed is null) to a velocity such that the opposing electro-motive force E, which it develops, becomes equal to the electro-motive force of the source. It is seen that the energy expended by the source of electricity diminishes from the time the motor begins to turn (curve I.); similarly, the intensity of the current (curve V.) diminishes to zero when E and E′ become equal. Curve II. represents the work developed by the motor at different speeds. Let us suppose these speeds are proportional to the electro-motive forces—a hypothesis easily verified in a well constructed magneto-electric machine—then we see, by the diagram, an augmentation of the work produced, up to a point where the speed of the motor becomes 50. At this moment the work done is at a maximum, and represents but 50 per cent. of the work expended by the source of electricity. The energy converted into work (curve III.) is equal to what is unconverted (curve II.). If the speed augments beyond this point the work produced (curve II.) diminishes, but the return augments (curve IV.).



The work produced and the return are hence perfectly distinct things which are too often confounded. There is no impossibility in making the motor return 80 per cent. of the work expended by the source of electricity, on condition you do not make this source produce all the work which it can furnish. When, at the limit, the work produced becomes null, the return becomes equal to 1. The same conclusion is arrived at on comparing curves I. and II. It is thus seen that energy not converted into work, diminishes more rapidly than the total energy expended by the source of electricity. When the motor is at rest, the work is zero, all energy being transformed into heat. When $\text{E}' = \dfrac{\text{E}}{2}$, the diagram shows that the work is equal to the loss; curves II. and III. cut each other at B and the return is 50 per cent. Several consequences result from this. If you wish to obtain the *greatest* results from any given source of electricity, the electro-motor, turning at normal speed, must be so arranged as to develop a counter electro-motive force equal to the half of the original source. If the *best* results are wanted greater speed is required, by which a return in work is gained with a corresponding loss in the quantity of work produced.

Curves III. and IV. show why an electro-motor heats more when stopped than when turning at a certain speed; the intensity of the current is greater in the first case than

in the second, the electro-energy not converted into work, diminishing with increase of speed, is converted into heat in the conducting wire. The two causes are correlative.

Let us cite a case having peculiar bearing on the transmission of power at a distance by electro motors, for instance, in electric traction on railways. Suppose our motor to turn at a normal speed developing a force of 70 volts. In this condition the work produced is represented (on the diagram) by A C, the work expended on the source of electric supply by A H, and the return is 0.70. If the existing work is augmented (by putting on a brake, for instance,) it will diminish the speed of the motor; but the curve II. shows that by this very diminuition of speed the work produced by the motor augments, and a new state of equilibrium is produced very close to the first. If, on the other hand, the resisting work diminishes, the speed will augment, and the work produced will diminish. Hence we see that the work of the motor augments with the resistance, and diminishes as well with it, a most favorable condition for regulating speed and maintaining it within certain bounds not far apart. This automatic governing is not to be found in any other motor. In the latter, special apparatus has to be called into play, as in the well-known case of steam.

This statement of the theoretical conditions affecting the functions of an electro-motor supplied from a given source, shows between what limits its different elements can be made to vary. The numbers which we have given for the maximum of work in batteries, as well as those given by M. Reynier in his work on the pile, have regard only to the total available energy in the external circuit, without consideration of the manner in which this energy is ultimately used. If, as in the above hypothetical case, it is desired to transform this energy into work of an electro motor, but half of the maximum work can be obtained. If, on the other hand, it is proposed to get the greatest *sum* of work in an indefinite time, the return can be augmented and collected up to as high as 80 and 90 per cent. of the energy represented by the expenditure of zinc in the battery, but then the pile does not produce its *maximum of work.*

The influences of the external resistances remain to be examined, such as are presented in transmitting force at a distance; also the resistance of the motor itself, and the practical returns obtained in certain special cases with motors of determinate type.

We will take occasion to recur to this subject after practical experience has had the last word. It is always well, however, to recall theoretical results, which never being altogether attained in practice, have an advantage in setting exact limits to our knowledge of what can be obtained from any given source of electrical supply; and. while destroying some illusions, proving some statements, which till now, have seemed too adventurous. (*La Lumière Electrique*, Aug. 7th.)

MULTIPLE SPECTRA.*

II.

I concluded my last article under the above heading with a reference to the case of carbon, and gave the results successively arrived at by Attfield, Morren, Watts, and others; these went to show that besides the line-spectrum of carbon mapped by Angström there exists a fluted spectrum of this substance.

Now comes my own personal connection with this matter.

In the year 1871,[1] I communicated to the Royal Society a paper in which the conclusion was drawn that the vapor of carbon was present in the solar atmosphere.

This conclusion was founded upon the reversal in the solar spectrum of a set of flutings in the ultra-violet.[2] The conclusion that these flutings were due to the vapor of carbon, and not to any compound of carbon, was founded upon experiments similar to those employed in the researches of Attfield and Watts, who showed that the other almost exactly similar sets of flutings in the visible part of the spectrum were seen when several different compounds of carbon were exposed to the action of heat and electricity. In my photographs the ultra violet flutings appeared under conditions in which carbon was the only constant, and it seemed therefore reasonable to assume that the flutings were due to carbon itself, and not to any compound of carbon, and this not alone from the previous work done in the special case of carbon, but from that which had shown that the fluted spectra of sulphur, nitrogen, and so forth, were really due to these "elementary" substances.

Professors Liveing and Dewar have recently on several occasions called this result in question. Professor Dewar, in a paper received by the Royal Society on January 8, 1880, writes as follows:

"The almost impossible problem of eliminating hydrogen from masses of carbon, such as can be employed in experiments of this kind, prove conclusively that the inference drawn by Mr. Lockyer, as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, *seeing that undoubtedly hydrogen was present in the carbon* used as the poles.

Subsequently, in a paper received by the Royal Society, on February 2, Messrs. Liveing and Dewar wrote as follows:

"Mr. Lockyer (*Proc. Roy. Soc.*, vol. xxvii. p. 308) has recently[3] obtained a photograph of the arc in chlorine, which shows the series of fluted bands in the ultra-violet, on the strength of which he throws over the conclusion of Angström and Thalèn, and draws inferences as to the existence of carbon vapor above the chromosphere in the coronal atmosphere of the sun, which, if true, would be contrary to all we know of the properties of carbon. We cannot help thinking that *these bands were due to the presence of a small quantity of nitrogen.*"

It will be seen that on January 8 Mr. Dewar alone attributed the flutings to a hydrocarbon, while on February 2 Mr. Dewar, associated with Mr. Liveing, attributed them to a nitrocarbon.

In fact in the latter paper Messrs. Liveing and Dewar published experiments on the spectra of various carbon compounds, and from their observations they have drawn the conclusion that the set of flutings which I have shown to be reversed in the solar spectrum is really due to *cyanogen*, and that certain other sets of flutings shown by Attfield and Watts to be due to carbon are really due to hydrocarbon.

As Messrs. Liveing and Dewar do not controvert the very definite conclusions arrived at by Attfield, Morren, Watts, and others, I can only presume that they took for granted that all the experimental work performed by these men of science was tainted by the presence of impurities, and that it was impossible to avoid them. I therefore thought it desirable to go over the ground again, modifying the experimental method so as to demonstrate the absence of impurities. Indeed I have started upon a research which will require some time to complete. Still, in the meantime, I have submitted to the notice of the Royal Society some results which I have obtained, which I think settle the whole question, and it is the more important to settle it as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views which appear to me likely to mislead, and which I consider to have long been shown to be erroneous. To these results I shall now refer in this place.

The tube with which I have experimented is shown in Fig. 1: A and B are platinum wires for passing the spark inside the tube; E is a small tube into which carbon tetrachloride was introduced; it was drawn out to a long narrow orifice to prevent the rapid evaporation of the liquid during the exhaustion of the tube. The tube was bent upwards and a bulb blown at C in order that the spark might be examined with the tube end-on, as its found that after the spark has passed for some time a deposit is formed on the sides of the bulb immediately surrounding the platinums, thus obstructing the light. After a vacuum had been obtained the tube was allowed to remain on the Sprengel pump, to which it was attached by a mercury joint for the purpose of obtaining a vacuum for a long time, in order that the last traces of air and moisture might be expelled by the slow evaporation of the liquid.

* Continued from p. 29.

[1] *Proc. R. S.* No. 187, 1878.

[2] The approximate wave-length of the brightest member on the least refrangable edge is 3881.0.

[3] That is, in 1878.—J. N. L.

The carbon tetrachloride was prepared by Dr. Hodgkinson, who very kindly supplied me with sufficient for my experiments.

On passing the spark without the jar in this tube, the spectrum observed consists of those sets of flutings which, according to Messrs. Liveing and Dewar, are due to hydrocarbon, and the set of flutings which is reversed in the sun, and ascribed by Messrs. Liveing and Dewar to cyanogen, also appears in a photograph of the violet end of the spectrum, Fig. 2. On connecting a Leyden jar with the coil and then passing the spark the flutings almost entirely vanish and the line spectra of chlorine and carbon take the place of the flutings without either a line of hydrogen or a line of nitrogen being visible.

As a long experience has taught me that these tubes often leak slightly at the platinums after they are detached from the pump, so that the evidence of such a *pièce justificatif* is only good for a short time, I took the occasion afforded by a visit of Dr. Schuster to my laboratory while the experiments wese being made to get my observations confirmed. He has been good enough to write me the following letter and to allow me to give it here:—

"*March* 21.

"MY DEAR LOCKYER.—The following is an account of the experiment which I saw performed in your laboratory on Monday, March 15:

"A tube containing carbon-tetrachloride was attached to the Sprengel pump. As exhaustion proceeded the air was gradually displaced by the vapor of the tetrachloride. The electrodes were a few millimetres apart. If the spark was taken without a condenser in the vapour the well-known carbon bands first observed by Swan in the spectrum of a candle were seen with great brilliancy; I also saw the blue band which you said was identical in position with one of the blue bands seen in the flame of cyanogen or in the spectrum of the electric arc. When the condenser and air-break were introduced this spectrum gave way to a line spectrum in which I could recognize the lines of chlorine. *The lines of nitrogen were absent, not a trace of the principal double line in the green being seen.* The hydrogen line Ha(C) was faintly visible when I first observed the spectrum, but it got gradually weaker and *finally disappeared altogether. When this line was no longer visible the condenser was taken out of circuit again, and the same carbon bands were seen as before.* These bands, therefore, show themselves with great brilliancy when a strong and powerful spark does not reveal the presence either of hydrogen or nitrogen. (Signed) ARTHUR SCHUSTER."

"March 21, 1880."

This result, which entirely endorses the work of Attfield and Watts, has been controlled by many other experiments. I have also repeated Morren's experiment and confirm it and I have also found that the undoubted spectrum of cyanogen is visible neither in the electric arc nor in the surrounding flame.

Hence then in the case of carbon, as in the prior cases of hydrogen, nitrogen and the like, those who hold that the flutings are due to impurities must, it would seem, abandon their position; for the flutings are undoubtedly produced by carbon vapor. Nor is this all; the suggestion that the various difficulties which have always been acknowledged to attend observations of this substance may in all probability be due to the fact that the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum, and that the tension of the current used now brings one set of flutings into prominence and now another, seems also justified by the facts. This suggests the view that a body may have a fluted spectrum of compound origin as well as a line spectrum.

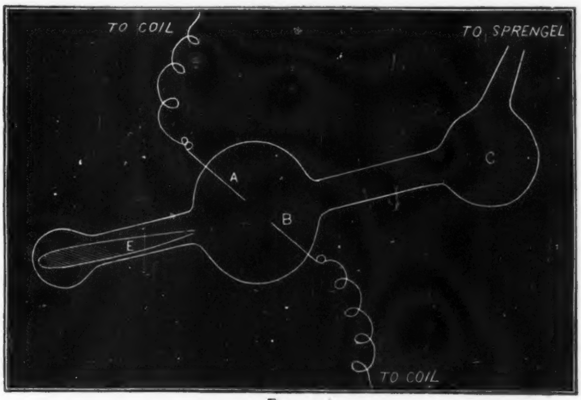


FIG. 1.

FIG. 2.

This conclusion is greatly strengthened by the preliminary discussion of a considerable number of photographs of the spectra of various carbon compounds.

A general comparison of the photographs first enables us to isolate the lines in the blue and ultra-violet portions of the spectrum (wave lengths 4300–3800) of the substance associated with the carbon in each case.

In this manner the lines seen in the photographs of the

spectra of CCl_4, $C_{10}H_8$, CN, CHI_3, CS_2, CO_2, CO, &c., have been mapped, and both the common and special lines and flutings thus determined.

The phenomena seen with more or less constancy are a blue line, with a wave-length of 4266; a set of blue flutings, extending from 4215 to 4151; and another set of ultra-violet flutings, which extend from 3885 to 3843 (all approximate numbers).

In a photograph of the spectrum of the electric arc (with a weak battery) between carbon poles in an atmosphere of chlorine, the blue flutings alone are visible, whilst, when the *spark* is similarly photographed, the ultra-violet flutings and the blue line (4266) are also visible, whilst the blue flutings become fainter.

From this we may assume, in accordance with the working hypothesis of a series of different temperature furnaces, as set forth in the paper of December, 1878 (see Fig. 3), that the different flutings and the line correspond to different temperature spectra, the blue flutings to the lowest and the blue line to the highest temperature, whilst the ultra-violet flutings occupy an intermediate position.

According to this working hypothesis there should be a series of horizons forming a perfect gradation between the spectrum which contains the blue line alone and that which contains the blue fluting alone (Fig. 4). In comparing the spectra of carbon under different conditions, I find this to be true. *The blue line never appears in conjunction with the blue flutings, unless the ultra-violet flutings are also present.* In other words, the highest and the lowest hypothetical temperature spectra are never visible together without the spectrum of the intermediate hypothetical temperature.

FIG. 3—Action of three different temperatures on a hypothetical substance, assuming three stage of complete dissociation.

But this is not all. By placing the spectra of the substances at different heat-levels, so to speak, I was enabled to construct a map, which not only indicates the mere presence or absence of the lines and their relative intensities, but shows a perfect gradation between the spectrum which contains the line alone and that which contains the blue flutings alone (Fig. 5). I would point out that there is nothing theoretical in this map. All the horizons depicted are copied from photographs of carbon under the conditions indicated, and theory has merely enabled me to arrange them *in order.*

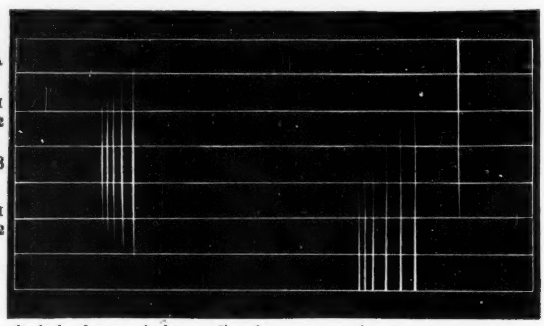


FIG. 4—Spectra of the hypothetical substance, in intermediate furnaces, assuming that the vapours are not completely dissociated.

This map I submit, therefore, bears out the hypothesis of differences of temperature indicated above, for it is seen that, while the blue line gradually thins out, the ultra-violet flutings appear first and grow in intensity. As these increase the blue flutings become visible, and further, as the

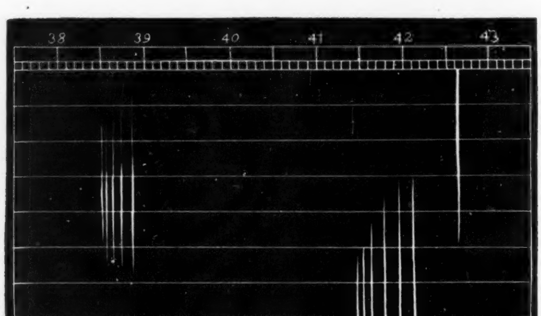


FIG. 5—The photographed spectra of some carbon compounds.

latter augments and the line disappears, the ultra-violet flutings gradually die out altogether.

It is philosophical to infer from these observations that not only are the line and flutings in question produced by carbon, but that the blue line (4266), since it is visible at the highest temperature, corresponds to the most simple molecular groupings we have reached in the experiments, and the flutings to others more complex.

The result to which attention is most to be directed in this place is that touching the two sets of flutings, and should future research justify the double conclusion (1) that these flutings are truly due to carbon, a result I accept, though it is denied by Angström and Thalèn; and (2) that the different flutings really represent the vibrations of different molecular groupings; a great step, and one in the direction of simplification, will have been gained.

Indeed it is much to be hoped that this ground will be at once worked over again by men of science who are both honest and competent: that the truth is sure to gain by such work is a truism.

I have so often taken occasion to refer with admiration to the work of Angström and Thalèn that I shall not be misunderstood when I say that their conclusions, to which such prominence is given, and on which such great stress is laid by Messrs. Liveing and Dewar, rest more upon theory and analogy than upon experiment.

Their work, undertaken at a time when the existence of so-called "double spectra" was not established upon the firm basis that it has now, and when there was no idea that the spectrum recorded for us the results of successive dissociations, gave, as I have previously taken occasion to state, the benefit of the doubt in favor of flutings being due to compounds, and it was thought less improbable that cyanogen or acetylene should have two spectra than that carbon or hydrogen should possess them.

Indeed, later researches have thrown doubt upon the view that the fluted spectra of aluminium and magnesium are entirely due to the oxides of those metals instead of to the metals themselves—and this is the very basis of the analogy which Angström and Thalèn employed.

The importance of the observations to which I have referred is all the greater because of the general conclusions touching other spectra which may be drawn from them. Thus from what I have shown it will be clear that if my view is correct, the conclusions drawn[1] by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands touching both the spectrum of magnesium and the spectra of comets, are entirely invalid. These conclusions are best given in their own words:—

"The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green bands of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn-out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connection the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon as it now appears, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum. . . .

"The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connection with conclusions to which our experiments point. Provided we admit that materials of the comet contain ready-formed hydrocarbons, and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum or of metallic lines. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature, and if nitrogen were also present that we should have the cyanogen spectrum as well. Either, then, the first supposition is the true one, not disproving the presence of nitrogen, or else the atmosphere which the comet meets is hydrogen only, and contains no nitrogen."

[1] Paper read February 12, 1880.

The importance of the question here treated of comes out very well from these two extracts. We find the same spectral phenomenon at once called into court, and very properly called in, both to suggest the existence of chemical substances of which the chemist has never dreamt, and to explain the chemical nature of a large group of celestial bodies.[1]

There is little doubt that when a complete consensus of opinion is arrived at among the workers, other suggestions more far reaching still will be derived from the prosecution of these inquiries. For the present, however, the chief point to bear in point is that both in line-spectra and in fluted spectra we have indications which I think favor the view that in each case the origin is compound rather than simple.—*Nature*. J. NORMAN LOCKYER.

OBAN, *July* 20.

[1] With special reference to this last question, that of cometary spectra, one of acknowledged difficulty, I may perhaps be permitted to add here by way of note that the view I put forward some years ago touching the relation to this spectrum to that of the nebulæ has been lately strengthened by the observation that at a low temperature one of the brightest lines in the spectrum of iron is that coincident with the chief line in the nebula-spectrum.

PHYSICAL NOTES.

FROM the above article we see that as far back as 1878, Mr. Lockyer communicated to the Royal Society a paper in which the conclusion was drawn that vapor of carbon was present in the solar atmosphere. This inference was founded upon experiments similar to those of Attfield and Watts, who showed that flutings are always present in different compounds of carbon exposed to the action of heat and electricity. This observation of Lockyer has been called in question by Liveing and Dewar, as they have found it an almost impossible problem to eliminate hydrogen from masses of carbon. This latter view has been long held by Edison, who, in a great number of experiments, some of which were participated in by Prof. Young, has found at the enormous heat developed by igniting a fine carbon thread $\frac{8}{1000}$ of an inch diameter, of high resistance, in air vacuum, until a light of 80 candles is reached, that only a carbon spectrum is given, until just a few seconds before the rupture of the loop, ***when a sharply defined hydrogen spectrum is observed***. On the other hand, in an observation of the purified spectrum of carbon tetrachloride, Mr. Lockyer (*Nature*, August 5th) found only carbon appeared at high temperatures. It is an excellent index of the spirit of unbiased investigation in the author of (*Nature*, December, 1878) The Hypothesis that the so-called Elements are Compound Bodies, and still later, of the Universal Hydrogen Hypothesis, to learn from Mr. Lockyer that, both in line and fluted spectra, he thinks we have indications which favor the view that in each case the origin is compound rather than simple.

IN a communication from William Huggins, F.R.S., received June 16th, 1880, and published in the *American Journal of Science* for August, are embodied some observations on the nature of the spectrum of water, which may give rise to a question of priority. It appears that Dr. Huggins made a photograph of the flame of hydrogen burning in air, December 27, 1879, but did not publish the fact.

On June last, Messrs. Liveing and Dewar state, in a paper read before the Royal Society, that they have obtained a photograph of the ultra violet part of the spectrum of coal gas burning in oxygen, and in a note dated June 8th, they add that they have reason to believe that this remarkable spectrum is not due to any carbon compound, but to water. Professor Stokes (whose well-known monograph in *Phil. Trans.*, 1852, has furnished so much suggestive material for others to work upon in this very line), authorizes the statement that Dr. Huggins, in a let-

ter bearing date 30th January, 1880, spoke of "a novel and interesting result," referring, probably, to the above-mentioned photograph. Since then, Dr. Huggins has taken a large number of photographs of the spectra of different flames, but only presents one (that of hydrogen) to the Royal Society. We regret this, both because of the loss to our general stock of science, in this unnecessary detention of the spectrum of carbon and its compounds, and because of the imminent probability of a repetition of these disagreeable questions of priority, as, on this side of the water (to the writer's knowledge), this particular subject is being eagerly studied under unique conditions.

The experiment of Dr. Huggins consists of first burning hydrogen *per se* in atmospheric oxygen, and then a mixture of oxygen with hydrogen in air. He finds the two spectra identical. For purposes of comparison, he very ingeniously photographs them on the same plate, in rapid succession, using the upper half of his spectroscope slit for the first, and the lower half for the second impression. As all the lines of both spectra fit each other exactly, without excess, it is evident that either represents the spectrum of water. The article referred to contains a partial spectrum, giving the characte ristic lines of wate.

PROF. J. TROWBRIDGE has recently studied the earth as a conductor of electricity and details some interesting experiments, and advances some bold speculations and prophecies in the *American Journal of Science* for August. In all the telephone circuits between Boston and Cambridge for a distance of about four miles, the ticking of the Observatory clock could be heard when transmitting time signals. This was attributed to the proximity of the telephone circuit wires to the time wires of the Observatory. Mathematical considerations. however, (Maxwell's Electricity and Magnetism, Vol. II., p. 209), will convince one that with telephones of the resistance usually employed, no inductive effect will be perceived between wires which run parallel to each other a foot apart for the distance of thirty or forty feet, even if ten-quart Bunsen cells be used. The transmission of these time signals is evidently not due to induction, but to tapping the earth, so to speak, at points which are not in the same potential. Running a wire five or six hundred feet long to ground at both ends, and putting a telephone in circuit, the ticking was distinctly heard when an exploration was made in an open field an eighth of a mile from the Observatory; yet the same wire, under similar conditions, gave no sound when one mile away from the central line between the Observatory and the Boston office. With the boldness of a Gallileo, Professor Trowbridge deduces thence the theoretical possibility of telegraphing across the Atlantic without a cable. He says: "Powerful dynamo-electric machines could be placed at some point in Nova Scotia, having one end of their circuit grounded near them and the other end grounded in Florida, the conducting wire consisting of a wire of great conductivity and carefully insulated from the earth, except at the two grounds. By exploring the coast of France, two points on two surface lines not at the same potential could be found; and by means of a telephone of low resistance, the Morse signals sent from Nova Scotia to Florida could be heard in France. Theoretically this is possible, but practically, with the light of our present knowledge, the expenditure of energy on the dynamo-electric engine would seem to be enormous."

A VERY curious observation has been made by M. J. Janssen of a remarkable inversion in a photographic image by exposure during different times. It passed from negative to positive with an intermediary neutral, invisible period. After a first exposure of $\frac{1}{1500}$ of a second a negative can be developed, a little longer exposure would dull the sharpness of the image; then there soon arrives a point where the negative disappears entirely. By a still longer exposure a new phase occurs, a positive image starts out from the plate, with lights and shadows just the reverse of the first and as sharply defined. By allowing further action of the light a second neutral condition occurs. M. Janssen does not say by what state this is followed.—*Moniteur Sci.*

M. SCHEURER-KESTNER in a note to the Académic des Sciences, qualifies a previous statement that sulphuric acid attacks platinum, by new experiments. Absolutely pure sulphuric acid does not attack platinum, but if there be ever so small a content of nitrous acid, a very appreciable quantity of the vessel is dissolved, $\frac{1}{10000}$ being enough for the purpose. In one of his experiments, on 60 grams of sulphuric acid, two milligrams of platinum were dissolved. This fact should be verified by manufacturers of concentrated sulphuric acid.

Mr. Albert Levy finds considerable variation in the ammoniacal contents of rain waters collected in the different quarters of Paris, but the annual means are identical. The per centages diminish from one month to the next, in passing from the cold to the hot season. The minimum at all stations was for the month of July, when there was present .93 of a milligram of nitrogen, against 1.35 in January. The potable waters of Paris are affected in exactly the same way. The reverse, however, is the case with the ammonia of the air which is most abundant in the hot season.—*Moniteur Scientifique*, Aug.

THE organisms described by Pasteur as the origin of epidemics and contagious disease, are so minute and few compared with the multiplying swarms of bacteria, etc., pervading all generating solutions, that it becomes necessary to provide a means of eliminating the masses of infusoria from solutions to be studied under the microscope. These microzoa haunt even the clearest drinking water at times, and it becomes highly important to easily determine their presence. M. Certes (Proceedings Acad. des Sciences), suggests the use of osmic acid as a sure means of killing them without destroying their tissues. He dips a glass rod into the solution to be examined and then into a 1½ per cent. solution of the acid; washing this in a narrow test tube of distilled water, it is easy to collect what is necessary for examination. There are certain precautions to be taken as to cleanliness and time of immersion. By the use of a mixture of Paris violet in diluted glycerine, he finds it possible by uniform difference of tint, to easily distinguish cellulose, amylaceous matter and the vibrating cilia.

M. DE LESSEPS, as an argument against the quarantine system, read a letter to the French Academy of Science, from the engineer in charge of the preparatory work of the interoceanic canal, informing him that a number of persons had disembarked at the isthmus while sick of yellow fever, without having propagated the disease among the workmen. Following this communication of M. De Lesseps, M. Bouley said he could not allow the inference from such remarks to pass unchallenged. Admitting that what M. De Lesseps said was true, that quarantines are a constant inconvenience to commercial and maritime relations, yet this injury is in the highest degree compensated for by the guarantees given to the public health. Since the international sanitary police has been watching over Egypt, and preserving it from the invasion of cholera by strict quarantine, this disease had come to be less feared in Europe. It is by quarantine alone we shelter ourselves from those diseases which vessels so easily carry with them, particularly the yellow fever to which M. De Lesseps refers. The atmospheric conditions which he says render quarantines nugatory, cannot contribute to the propagation of epidemics, unless those who are attacked are allowed to land from the vessels which contain the germs. But these germs are not intangible exhalations, subtile vapors, effluvia which have a property of fatal expansion, against which we can do nothing. Quite the contrary is true. Thanks to the researches of experimental science, the principle of contagion is no longer unknown; it has taken body and can be studied and followed in its manifestations. But even before this accession to our knowledge, practice, inspired by observation, had proved that strict surveillance of men and things coming from suspected countries would prevent the spreading of the germs. This is the province of the quarantine and by it alone can it be done. It is, then, necessary to maintain it in spite of the convenience to commercial and maritime relations.

OTTO A. MOSES.